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In Defence of Genetics

By J. B. S. Haldane

IT is of the utmost importance that biologists in this country should be able to appreciate both the positive and the negative elements in the views put forward by Lysenko and his supporters in the Soviet Union. Unfortunately, this has been made much more difficult by ill-informed criticism of genetics by supporters of Lysenko in this country. If geneticists held the views attributed to them, they would doubtless deserve severe criticism. As they do not hold them, such criticism can only make a just appraisal of Soviet genetics more difficult.

It will be convenient to take as my text the original form of an Educational Commentary issued on behalf of the *Daily Worker*, and later emended. According to this document, "the modern doctrine of genetics flatly rejects the belief that plants and animals can pass on to their descendants characters acquired under the influence of their conditions of life." This is justified by a quotation from Weismann over sixty years old.

Some textbooks of genetics contain dogmatic statements to this effect, though they also contain accounts of the effects of colchicine, which induces characters in plants which reappear in their offspring for an indefinite number of generations. It is also quite certain that changes of this type, including the permanent loss of organs by disuse, can be induced in single-celled plants and animals. The greatest American student of protozoan genetics, Jennings (1935), wrote as follows: "The facts in the Protozoa show that after the genic materials have undergone an adaptive change, they may assimilate and reproduce in the changed condition, resulting in an inheritance of the change. . . . There is thus no general reason why it should not occur in higher organisms; no *a priori* reason why the germ-cells of higher animals should not thus acquire certain adaptive characteristics, and hand them on to descendants. Whether they do so or not is simply a question of fact, to be determined by observation." If this is dogmatic Weismannism, the Pope is an agnostic. It is perhaps worth adding that Jennings was a founder of the American journal *Genetics*.

If I may be allowed to quote my own work (Haldane, 1941), I stated: "We can regard the gene as an organ in the cell, just as the heart, pancreas, or femur is an organ in the body as a whole."

I can only add that, if I had thought that genes were never altered as a result of their activity or inactivity, I chose a remarkably bad analogy, and that in spite of this, so far as I know, no geneticists have attacked my opinion.

As, of course, Lysenko agrees, acquired characters are not usually inherited. He refers (p. 87) to "the frequently observed phenomenon when the altered organs, characters, or properties of an organism do not appear in the progeny."

There is, however, a sense in which the commentary is quite correct in its statement. Until I read Lysenko's speech, I had not recognised the idealistic character of Mendel's formulation of his results. He spoke of the transmission from one generation to another of differentiating characters (*differierende Merkmale*). Now, in ordinary speech we do speak of the transmission or inheritance of a character. For example, I may say that I have inherited my father's watch, and also his eye colour or baldness. A geneticist should not use such language, and does not if he is a good geneticist. A character is not something which can be detached and handed on like a watch. Mendel was presumably a Thomist, and his *differierende Merkmale* are merely St. Thomas's *formae substantiales* in lay dress. I had given (1941, p. 20) as my ground for rejecting the "unit character" theory that it was too mechanistic. On both of these grounds it must be rejected. What is inherited is not a set of characters, but the capacity for reacting to the environment in such a way that, in a particular environment, particular characters are developed. It is therefore incorrect to speak of the transmission of a character, whether "acquired" or not.

The question is more properly formulated like this. An organism X manifests the character-α in environment A, β in environment B. If it remains in environment A, its descendants Y will not, in general, manifest the character β in environment A. But in some cases if X has been placed in environment B, Y will show the character β even in environment A. The question to be decided is how often, and in what circumstances, this occurs.

The Commentary goes on to state that "the modern doctrine of genetics considers the hereditary process as something separated from and independent of the living body as a whole. Weismann put forward the theory that the living body is divided into two parts, a mortal body (or soma) and an immortal hereditary substance or germ-plasm, which existed within the mortal body, but independent of the latter. In his view and that of his followers,

changes in the living body could have no effect on the hereditary substance, which is reproduced from one generation to another unchanged by the conditions of life of the parent organism."

Modern geneticists believe that the hereditary process depends on material objects, called genes, in the nucleus of a cell, and on other objects outside the nucleus. Usually they reproduce their like, so that a cell containing a gene of a certain type divides into two others each containing a gene of this type. Geneticists are not sure whether one of these two is the original gene and the other a copy, or whether the original one is scrapped, and two similar ones produced. In the second case the genes, so far from being immortal, die at each cell division. In the first a gene may persist for some time. But since a bull consists of about a thousand million million cells, and may produce several million million spermatozoa in its life, the chance that a gene which it receives from its father is handed on to a calf is probably less than one in a million million. A precarious kind of immortality!

Not only are genes not immortal, but they do not always reproduce their like. The production from one gene of a different type of gene is the event called mutation. Some genes are so faithfully reproduced that, in a suitable environment, all but less than one per million of the offspring receive a similar gene. Others are so frequently altered that this frequency rises above 1 per cent. Both the frequency of mutation and the direction of the change depend on the environment. Mutation frequency can be increased by temperature changes, by abnormal food, by various chemicals including antibodies, by X-rays, by hybridisation, and so on. The extranuclear bodies which play a part in the hereditary process, and some of which, at least, can be transmitted by grafting, seem to be still more easily affected by environmental changes.

Thus changes in the rest of the body can and do affect the genes, and the genes certainly affect the rest of the body. This is how they are detected. The two are in no way independent, though they have a relative independence, just as a change in the femur may have no marked effect on the heart, or conversely. The genes, like everything else, are a union of opposites. If they were at the mercy of every environmental change, heredity would be impossible. If they always reproduced their like, evolution, and even the production of domesticated animal and plant varieties, would be impossible. Ninety-nine geneticists out of a hundred would agree to this statement. In the first twenty years of this century a number of

geneticists believed in the immutability of genes. I do not know of any who do so to-day.

The Commentary continues as follows: "Such variations of types as do occur from one generation to another (e.g. the variations between grey and white mice) are simply the result of mutations and of the interchange of and recombination of microscopic bodies known as genes in the process of reproduction." (In fact, very few if any genes are visible with a microscope.) Even Weismann did not believe this. If you take seeds from a gorse bush and grow them under a bell jar in damp air they develop leaves, but no spines. If you keep rabbits of certain breeds warm they grow up white, if you keep them cool they darken, and so on. Geneticists are, however, agreed that most, if not all, variation, in so far as it is not determined by the environment (and most economically important variation is determined to some extent by the environment) is determined in the way described in the Commentary. As the laws of interchange and recombination of genes are fairly well known, and those governing mutation are being worked out, this means that we can and do produce animals and plants of the type desired.

This is not a matter of combining "characters," which are metaphysical conceptions, but of finding out how genes, which are material objects, act. To take a trivial example, I wanted a yellow cat without stripes, and was able to produce him from a particular mating, although no such cats were previously known, because I knew enough about the action of certain genes to predict that this mating would produce him, although neither of his parents was yellow. Unfortunately, in England I cannot work with anything larger than a cat.

The Commentary continues: "Thus in the hands of the modern geneticists, genetics has become a science of statistical probabilities, that is of estimating the chances that certain mutations and combinations will occur, rather than of trying to discover any laws of causal connection which could help mankind to modify and control nature."

To go back to my yellow cat, by making the mating which I did, I put up the chance of getting the sort of cat I wanted from less than one in a million to one in four. We do not yet know enough to put it up from one in four to certainty. But putting it up to one in four is enough to allow us to control nature.

It is utterly untrue that geneticists do not try to discover laws

of causal connection. Let me take a typical example. It is known that over 1 per cent. of the babies of mothers who lack a certain antigen in the blood by men who possess it, die of jaundice before or soon after birth. This is a mere statement of a chance. The next step was to discover that the deaths were due to the presence of an antibody in the mother's blood, which leaks through the placenta and kills the baby. Once this antibody is found, we can give the baby a blood transfusion at birth, often before it develops jaundice, and save its life. In future we shall probably be able to remove the antibody from the mother's blood, and save those babies which develop jaundice before birth.

It is clear that those who make such statements about genetics are unaware of the existence of numerous books on physiological genetics which are entirely concerned with the function of genes, and not at all with their statistical distribution. But such statements can only serve to antagonise geneticists in Britain whose work is ignored or traduced, and thus to discredit Soviet genetics.

It is, of course, true that Weismann believed in random variation. Here is what Bateson wrote about him in 1905: "Variation, all agree, is going on still. Why not look and see if it is at random? Unfortunately for Professor Weismann's philosophic scheme, this is now being done. . . . If thirty years ago it could be conjectured in ignorance that variation was chaotic, many know better to-day." More modern geneticists hold the same view. "Judging from these results, therefore, the mutation process does not proceed at random; it is pre-determined, can be controlled by altering the cell environment in a definite manner," wrote Gustaffsson (1947).

With these preliminaries, I may state my own position. I am a Darwinist, although Darwin (1879) wrote: "Man, like every other animal, has no doubt advanced to his present condition through a struggle for existence consequent on his rapid multiplication, and if he is to advance still higher, it is to be feared that he must remain subject to a severe struggle. Otherwise he would sink into indolence, and the more gifted men would not be more successful in the battle of life than the less gifted." Similarly, I am a Mendelist-Morganist, although Mendel used an idealistic terminology, and Morgan wrote of the mechanism of heredity. But Morgan and his colleagues made the very great advance of showing that heredity has a material, not a metaphysical, basis. Their discovery underwent the normal fate of all advances towards materialism. It was mechanistically interpreted. And it is often taught in a manner

which combines mechanism and idealism. Thus geneticists sometimes say that an animal has the same gene as its father in a given locus, as if genes combined the property of indestructibility with the still more remarkable one of being in two places at once. We should certainly combat such tendencies. But that does not mean that we should reject the large element of genuine, constructive, materialism in Morgan's views.

The hypothesis that genes, or chromosomes, are the only structures concerned in genetics is certainly untrue. Since Correns' work in 1902, it has been quite clear that structures outside the nucleus played an important part in heredity in plants, and in the last fifteen years similar cases have been found in animals, notably in *Drosophila* by l'Héritier and Teissier, and in mice by Little and his colleagues. Like the extranuclear factors studied by Michurin in plants, these can be transferred from one organism to another by non-sexual means. The *Drosophila* factor is transmissible by grafting. The factor in mice is particularly interesting. It is found in milk, and on being drunk by the new-born mice, finds its way into their mammary glands, where it multiplies, causing increased cell growth, and in later life frequently cancer. It seems entirely possible that milk transmission may prove valuable in improving races of mammals, as grafting can be used in plants.

It must be emphasised that a belief in Mendelism does not mean a belief that all inheritance is Mendelian or chromosomal, any more than a belief in polar bonds in chemistry implies a disbelief in non-polar bonds. Personally, I was writing of non-Mendelian inheritance in 1924. In the same year Bateson, the apostle of Mendelism in Britain, wrote: "As to what the rest of the cell is doing, apart from the chromosomes, we know little. We think that in plants the presence or absence of chloroplasts may be a matter of extra-nuclear transmission. Perhaps the true specific characters belong to the cytoplasm, but these are only idle speculations." We know a good deal more now.

It has been said that the whole theory of genetics has been so strained by the attempt to incorporate new knowledge that it will break down, and we had better admit it. I think that a Marxist should be the last person to admit it. Genes exhibit a good deal of stability in their reproduction, otherwise heredity would be impossible. They do not exhibit complete stability, or evolution would be impossible. They behave as units in certain contexts; in others they do not. It is not very easy to alter the germ-cells so

that later generations differ genetically. When you do you get very striking results, as when Humphreys, after transforming the ovaries of salamanders into testes by grafting, got broods containing 100 per cent. females. Every science is at first based on easily reproducible experiments, whose results are then exalted into "laws of nature." But we do not cease to believe in atoms because they can be split. Nor need we cease to believe in genes because they can be changed. On the contrary, if they were unchangeable, I, as a Marxist, could not believe in them.

Lysenko's most striking claim is the production of autumn wheats from spring wheats, and conversely, by alterations of temperature and other conditions over several years. If this claim is accepted—and the result has been repeated so often that it would be very rash to reject it—it seems reasonable to point out that the germ-cells of future generations are descended directly from the cells of the growing point of the young shoot. It is precisely these cells which are affected by the process of vernalisation. And it is not surprising that when they are changed the germ-cells are also changed, whereas it is very much harder to induce changes in future generations by changing the metabolism of leaf or root cells.

For this reason, I am sceptical of the claims that in general "acquired characters are inherited." It is, of course, true that you cannot get a race of pigs which fatten rapidly unless you feed your pigs well for a number of generations. There is no way of picking out the pigs which would fatten if well fed, so long as the food is not there. And to my mind it is ridiculous to suppose that all the genes, and perhaps cytoplasmic factors, responsible for fat production under good feeding, were there in the wild ancestors of the pig, and that breeders have merely combined them. The question is whether the high diet makes the genes (or any other material basis of heredity) change in such a way that the pigs lay down more fat. It might just as well have the opposite effect, and cause them to oxidise their food more rapidly. My own view is that changes in the physical basis of heredity are largely due to internal conflicts in the cell. One of the most potent methods of producing them is by hybridisation of species or races so distant that they can only be crossed with difficulty. This does not merely give new combinations of genes of the pre-existing type, but changes to genes of a new type. Some of these changes can be valuable to man.

Lysenko and his supporters point with justifiable pride to the very great increase in productivity which has occurred in many parts of their country in recent years. If I did not regard its economic system as superior to that of my own country, I should be forced to suppose that its methods of livestock improvement were greatly superior. But I am much more convinced that collective farming is superior to capitalist farming than that Soviet breeding practice excels our own. If, of course, they can produce more wheat per acre, or more milk per year from a cow of given weight, than the best British or Danish farms, I shall have to change this opinion.

Certainly, however, we have a great deal to learn from Soviet geneticists. We must realise that there is a lot of quite unjustifiable idealism and mechanism in our basic concepts. We must also take more cognisance of extra-nuclear inheritance and the possibilities of grafting. Here, as it happens, animal geneticists outside the Soviet Union can produce more striking experimental results than any from the Soviet Union which are accessible in this country, though as regards plants the opposite is the case. We must carefully study the results of Soviet experimental work as it becomes available. But it must be realised that the results of experimental work are not available until they are published in such a form that they can be repeated. Every step to make such work accessible is a major contribution to good relations with the Soviet Union.

Most British geneticists are, of course, seriously handicapped by their divorce from practical agriculture. This will make it exceedingly hard for us to verify some of the principal claims of our Soviet colleagues.

We must also beware of idealistic interpretations of anti-mechanist tendencies in Soviet biology, of which Shaw has given a good example in a recent article. In a recent discussion in London, some Marxists went so far as to deny that there was a material basis of inheritance. There is good reason to doubt that any parts of a cell are *only* the material basis of heredity. Genes certainly play an active part in a cell's ordinary life. But a Marxist can no more deny a material basis for heredity than for sensation or thought.

If this discussion were merely academic, I might well keep out of it, as others in similar positions have done. But if the views held in Marxist circles are going to be of increasing importance in Britain in the future, as they have been in other countries, the

situation is different. I believe that wholly unjustifiable attacks have been made on my profession, and one of the most important lessons which I have learned as a Marxist is the duty of supporting my fellow workers. We are not infallible, but we certainly do not hold many of the opinions which are attributed to us.

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The Biological Controversy in the Soviet Union and Its Implications

By J. D. Bernal

THE importance of the genetics controversy in the Soviet Union ranges far wider than the field of biology. It is already being presented as a political rather than a scientific controversy and has become a major intellectual weapon in the cold war. It is this wider aspect of the controversy that makes it possible and even desirable that it should be discussed by others than professed biologists who are inevitably influenced by the very tradition of genetics that is in question.

The duration of the controversy inside the Soviet Union and the violence of its effects outside show that the whole matter is one of an importance that demands that it should be understood by all who are concerned with the main political and philosophical problems of our time. That understanding has now become much simpler since the publication in English of the verbatim account of the discussion at the Lenin Academy of Agricultural Sciences in August of last year.[1] There is no doubt that this publication will, when it is assimilated, give rise to a new flood of attacks on science in the Soviet Union, and by the usual implications on everything else there. The way in which these attacks are taken up in the press normally so indifferent to science and are amply disseminated by the B.B.C., with the accompanying distortions and inferences, make it all the more important to study the book itself. Like every other political event in the Soviet Union, the controversy has been hailed as evidence of both wickedness and folly in the conduct of the Socialist state. It has been claimed as a blow to the liberty of science, as a turning back to confused and antiquated ideas, and as certain to result in the destruction of Soviet science and in the rapid decay of its agriculture. The enemies of the Soviet Union, for lack of more effective means of injuring it, have, however, been proclaiming its doom for internal reasons for a very long time, and the event has, unfortunately for them, always proved to be the opposite of their predictions. Even those who are still certain that the Bolsheviks are knaves might by now be accustoming themselves to the idea that they are not fools as well.

[1] *The Situation in Biological Science* (Foreign Languages Publishing House, Moscow, 1949), (Collets, London).